Response to Mikhail Verbitsky comments (CC1 CC2)

Dear Mikhail Verbitsky,

Thanks for your comment. We hereafter respond point by point:

CC1

Simple but physics-based models of ice ages have been introduced many years ago (e.g., Birchfield and Weertman, 1978; Chalikov and Verbitsky, 1984, *etc.*); nevertheless, phenomenological models of glacial rhythmicity are, as Michel Crucifix (2012) said, still “seductive”. Their obvious lack of underlying physics is compensated by introduction of artificial thresholds and other Boolean statements that, indeed, allow them to reproduce empirical data but do not add much to our understanding of the physical nature of glacial periods. Therefore, every attempt to introduce “A simple physical model for glacial cycles” should be welcomed.

Unfortunately, the presented paper has (I frankly hesitate to say that, but for the lack of a better word…) a flaw that needs to be addressed before one proceeds to the results.

You approximate ice discharge as q = v H/L, where L is a constant. Even intuitively, making the horizontal size of ***evolving*** Northern Hemisphere Pleistocene ice sheets to be a constant, is strange, but from the basics of ice physics it is simply incorrect: H and L are not independent, and H is proportional to L^(1/2) (e.g., Verbitsky, 1992, Bahr et al, 2015). Taking this into account may dramatically change the dynamical properties of your governing equation (1).

We understand your concern here and would completely agree except that L, in equation (1), does not represent the horizontal size of the ice sheet. We recognize that our current designation of L in that equation is incorrect and has understandably misled you. Indeed, in order to estimate the divergence of the flux between two horizontal points of an ice sheet, the local slope should be estimated by the inherent relationship between H and L (a given profile). It would therefore be incorrect to use a constant L. However, this is not what we are doing here. Under our (spatially) adimensional approach what Equation (1) says is simply: “The evolution of ice thickness (intended to be characterizing the mean state of the whole ice sheet) is the surface mass balance minus the ice discharge into the ocean”. The latter would be captured in a 3D model by the ice flux at the grounding line, which is herein written as $v \cdot H/L$, where L should be understood as a scale adjustable parameter and represents the length of the boundary between the ice sheet and the ocean, hereafter L_{ocn}. To avoid misunderstanding, we will replace L in this equation by L_{ocn} and the discharge will be

$$q = v \cdot \frac{H}{L_{ocn}} \quad \text{(Eq. CC1.1)}$$

However, we realize that when we introduce the dimensionalization of the horizontal derivatives, employing just H/L with a fixed L can be problematic since the driving stress becomes quadratic with the ice thickness (equation 10). This is clearly not the case in real ice sheets (e.g. figure 4b from

Morlighem et al., 2013). Thus, we will include in the text a variable L in Equation 10, following your recommendation of using

$$L = c \cdot H^2. \text{ (Eq. CC1.2)}$$

We have now implemented these changes in our model. Below follows a brief diagnostic and prognostic analysis of these changes. We use a range of coefficients c in Eq. (CC1.2) from 0.2 (based on Vatnajökull ice cap) to 0.9 (based on Antarctic ice sheet). As can be shown in Figure CC1.1 below, in the case with fixed L, the driving stress leads to very high values, only exceeded for the extreme value c = 0.2 in about half of each cycle.

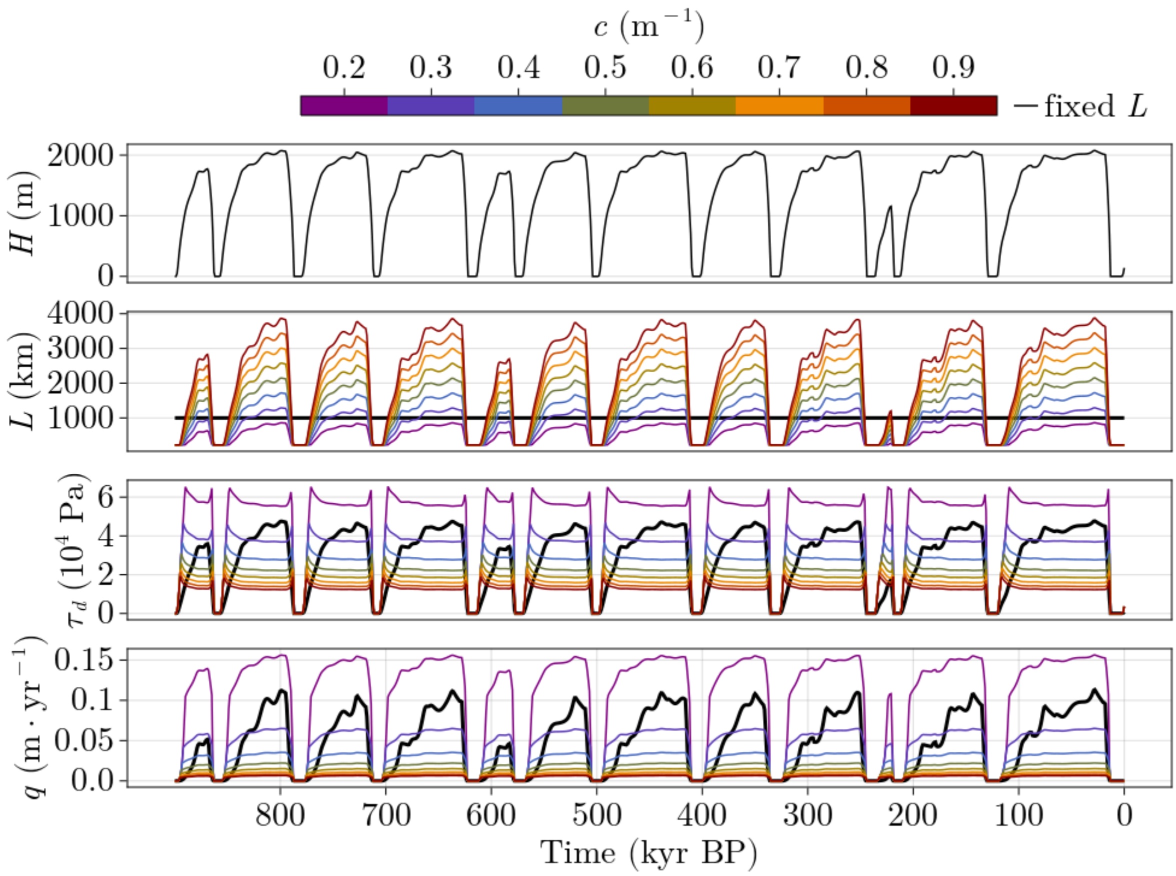

Figure CC1.1. Diagnosed L, τ_d and q for a fixed H evolution. Colors indicate different values of the coefficient in $L = c \cdot H^2$. Note that the black line corresponds to the case in which we kept L constant.

We next show the effect of these changes on the AGING configuration presented in our manuscript for c = 0.9. As shown in Figure CC1.2, we can converge to the previous results, now with a dynamic L. The proposed changes in ice dynamics only slightly displace the parameter space. Therefore, we can say that despite the fact that we will need to recalibrate the simulations shown in the paper, our conclusions remain unchanged. We thank you for giving us the opportunity to correct it, and kindly invite you to proceed to the reading of the results and following sections.

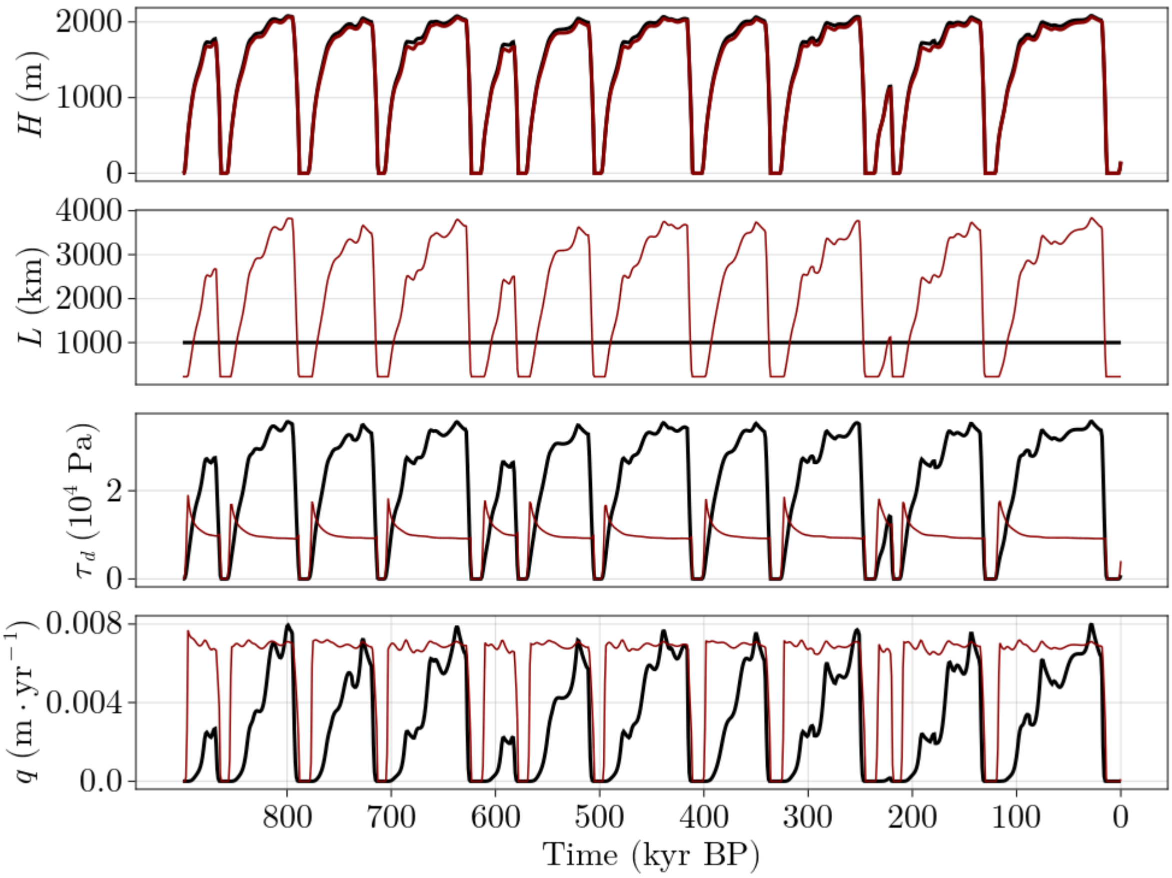

Figure CC1.2. Simulations using AGING configuration for $c = 0.9$ (red) and our previous results with fixed L (black).

Few comments which are less significant, but may be you consider them helpful:

1. Your model is not "adimensional" as you claim in your PACCO abbreviation. For example, your equations have dimensional H, measured in meters, and dimensional time, measured in seconds. You didn't attempt to make your system adimensional. You simply do not resolve space.

You are right in saying that PACCO is not an "adimensional" model but rather a "spatially adimensional" or a "zero-dimensional" model. We will clarify this in the revised version of the manuscript.

2. It would be very helpful, if you present your final equations all together in one place instead of forcing a reader like me to do this work and substitute let say tau_d into v_d into v into q into dH/dt. At this point, the model description looks like a verbalized computer code (that I suspect it is).

The gradual increase in complexity that structures the paper is fully intentional and aims to show the influence of each process on the periodicity and shape of the glacial cycles. We believe that gathering the equations as you suggest is in fact much closer to a “verbalized computer code” than our approach, which attempts to guide the reader through the model in a practical way. Additionally, we have to note that describing all the terms that participate in the ice dynamics (for example) in separated equations is the common practice. Otherwise, the following examples of the literature (e.g. Cuffey and Paterson (2010); Huybrechts and Oerlemans (1988); Quiquet et al. (2018); Pattyn (2017); Robinson et al. (2020)) should also be considered as “verbalized computer codes” (that we suspect are not).

3. Since you position your model as a physical model, it would be nice if you explain the basic physical nature of the governing equations. For example, I was able to figure out that your equation (1) is scaled kinematic condition on the ice sheet free upper surface, but I am not sure I can explain with the same confidence equation (29) especially when Q is not defined.

We will make sure to describe this properly in the revised versions of the manuscript. On the other hand, we will address your question about Equation (29) in CC2 below.

4. Line 35: “However, most of them rely on very mathematical approaches and include artificial or imposed thresholds and trends (Paillard, 1998; Paillard and Parrenin, 2004; Gildor and Tziperman, 2001; Verbitsky et al., 2018; Ganopolski, 2024).” First, what “very mathematical” means? And, second, I am not aware about “artificial or imposed thresholds and trends” in Verbitsky et al (2018).

In the revised version of the manuscript, we will refer more explicitly to the articles assuming the existence of thresholds and trends, which is what we understand to be a mathematical approach. In this context, we understand that "very mathematical" is not the right term and will resolve this in the revised version of the manuscript.

We emphasize that most of the processes resolved in those models are specific to the proposed hypothesis to explain glacial-interglacial variability. In contrast, PACCO solves a whole set of physical processes, covering various hypotheses and allowing to compare competing processes.

5. And finally, a bit of funny thing. Your introduction begins with the sentence that makes a reader to believe that Paillard in 2001 and Ganopolski in 2024 introduced glacial-interglacial variability to the world. With all due respect to celebrated scientists, I would suggest someone like Agassiz to be mentioned there. It would also help to explain (next sentence) how Milankovitch was able to offer his theory before them.

We kindly thank you for your suggestion. When we wrote the manuscript we did not intend to do a complete summary or review of the history of glacial cycles, but in fact it would be a nice addition to the introduction.

Respectfully,

Mikhail Verbitsky

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CC2

I think I owe you a more explicit explanation of my discomfort with Equation (29).

It is a common knowledge that for typical ice sheets the Peclet number (Pe) is of order of 10. This means that temperature advection dominates heat diffusion and an ice-flow trajectory has a near-constant temperature determined by its value on the upper free surface of the ice sheet (e.g., Grigorian et al, 1976, Morland, 1984, etc). The thickness of the basal boundary layer where, instead, the heat diffusion begins to dominate, is proportional to Pe^(-1/2)*H and is about 100 m. The timescale of the upper-surface temperature "delivery" to the basal boundary layer is the same as the timescale of ice growth. Your equation (29) seems to describe the heat balance of such basal boundary layer. Its thickness H_b = 10 m implies that you assume Pe to be even larger than 10. Nevertheless, the mechanism of delayed cold ice delivery to the bottom layer is absent in equation (29) and replacing it with conduction term (34) is very difficult to justify. Obviously, the absence of the vertical-temperature-advection timescale may have significant implications for the entire system dynamical properties.

Respectfully,

Mikhail Verbitsky

References

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Thank you very much for clarifying your concern about Equation (29). Our approach was to keep the thermodynamics as simple as possible in order to characterize how different temperate base timescales influence the basal dynamics in the ice sheet. Thus, we assumed a heat balance between heat fluxes into the base (h_{geo} and h_{drag}) and out of it. Therefore, we assumed cooling given by the temperature difference between the atmosphere and the base (h_{cond}). Note that here, we understand "base" as a layer of thickness H_b . We were perhaps a bit careless when assigning to it a fixed value of 10 m without checking the Peclet number relation $H_b = Pe^{-1/2} \cdot H$, which would indeed yield a much larger value. We thus acknowledge your comment. Lastly, we had eliminated the advection term because its contribution was negligible and because we understood that its effect was accounted for by h_{cond}, in the sense that the air temperature needs more time "to be delivered" to the base if the ice sheet is thicker. However, we understand that your more rigorous approach needs to be taken into account. Therefore, in the revised version of the paper we will explicitly include an advective term, h_{adv}, and change Eq. (30) to

$$\frac{dT_{\mathrm{ice}}}{dt} = \frac{Pe^{1/2}}{c_{\mathrm{ice}} \cdot \rho_{\mathrm{ice}} \cdot H} \cdot (h_{\mathrm{geo}} + h_{\mathrm{drag}} + h_{\mathrm{cond}} + h_{\mathrm{adv}})$$

where

$$h_{\mathrm{cond}} = k_T \cdot \frac{T - T_{\mathrm{ice}}}{H \cdot (1 - Pe^{-1/2})},$$

$$h_{\mathrm{adv}} = c_{\mathrm{ice}} \cdot \rho_{\mathrm{ice}} \cdot \dot{s} \cdot \frac{T - T_{\mathrm{ice}}}{Pe^{1/2} - 1},$$

and by definition,

$$Pe = \frac{H - H_b}{k_T} \cdot \rho_{\mathrm{ice}} \cdot c_{\mathrm{ice}} \cdot \dot{s} \simeq \frac{H}{k_T} \cdot \rho_{\mathrm{ice}} \cdot c_{\mathrm{ice}} \cdot \dot{s}$$

Note that we removed the dependence on H_b since it entails a second order variation on the heat fluxes (given that the dependency of $$H_b$$ on the Peclét number is already contained, e.g. $$Pe^{-1/2}$$ in the denominator) and we let the Peclet number evolve according to H and the vertical velocity scale, that is assumed to be the snowfall $\dot{s}$.

We show in Figure CC2.1 below the THERM configuration applying the new equations for the same set of parameters. We have taken the advantage of improving the thermodynamics, following your suggestion, so we can now use a value of h_{geo} of 50 mW/m² (instead of the former 5 mW/m²), much closer to observations for North America and Europe. The 100 kyr periodicity is produced but still lacks appropriate time and shape. Thus, our conclusions regarding thermodynamics do not substantially change. The reason for this is that, under the THERM experiments, what ultimately controls the timing and amplitude of the deglaciation, is the required time for the base to become temperate, allowing the enhancement of basal sliding. With our former thermodynamics we were already exploring the phase space of this phenomenon (mainly through different values of $$tau_\mathrm{kin}$$). Yet we very much appreciate your comments and suggestions which we believe are contributing to improve our model.

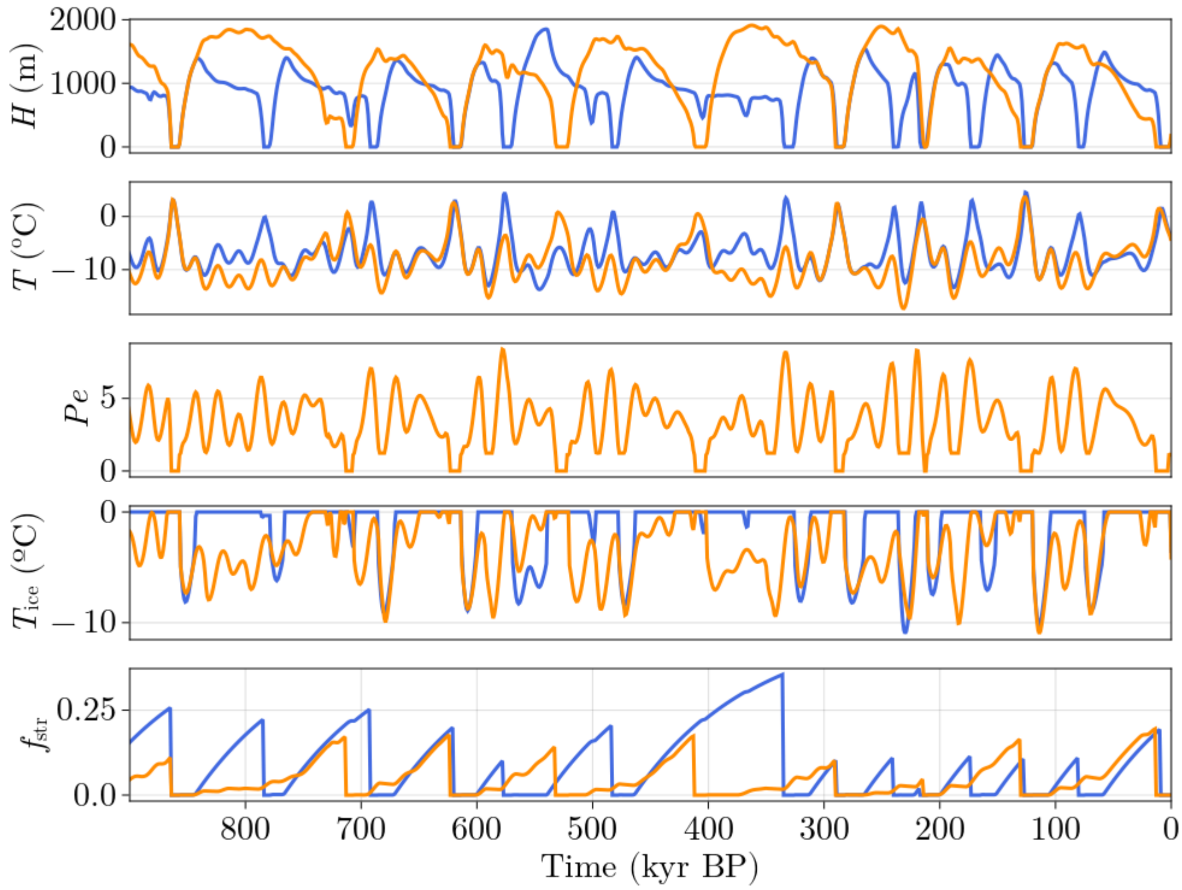

Figure CC2.1. Comparative plot between the old (blue) and the new (orange) THERM configuration.

We now summarize the main changes that we consider to be implemented in the document:

1. We will add a variable for the typical horizontal spatial scale of the ice sheet following your suggestion $L = c \cdot H^2$ for the driving stress (Equation 10). However, we will keep L_{ocn} as a constant when calculating ice discharge (Equation 3).
2. We will clarify some terms and add some references that we consider relevant thanks to your comments.
3. We will modify the thermal balance in the basal boundary layer equations.

Finally, we would like to emphasize again that even though these comments are useful and have changed a few things in the manuscript (requiring recalibration in some cases), our results are qualitatively very similar and thus the conclusions are robust.

We hope our answers clarify your concerns.

Best regards,

Sergio Pérez-Montero et al.

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